Thomas Ramos

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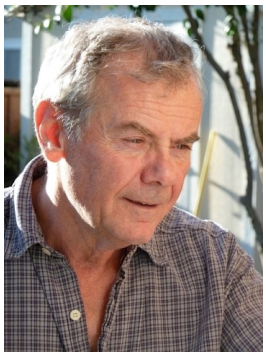

Courtesy: Thomas Ramos

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▼ ABSTRACT

In this interview, David Zierler, Oral Historian for AIP, interviews Thomas Ramos, a physicist detailed to the Principal Associate Director for Weapons and Complex Integration at Lawrence Livermore Laboratory. Ramos discusses his current work writing an unclassified history of the weapons program at Livermore and the broad perspective this has given him on the Laboratory from the postwar era to the present. Ramos recounts his childhood in Brooklyn and his military enlistment after high school, which led to a tour in South Korea and then an order from West Point to pursue a master's degree in nuclear physics. He discusses his graduate work at MIT and his research on bubble chamber experiments at Fermilab and Argonne before being ordered back to West Point to teach nuclear science. Ramos describes the opportunities leading to his appointment at Livermore four years later and his initial work on the X-ray laser program and the origins of the SDI program. He discusses the impact of the end of the Cold War on the Laboratory and the extent to which Reagan's military spending accelerated the Soviet collapse. Ramos discusses his work at the Pentagon as a legislative affairs officer for the Assistant Secretary of Defense for Atomic Energy, and he explains Livermore's increasing involvement in monitoring nuclear proliferation among terrorist groups and rogue states. He describes his transition to counterproliferation as a result of the end of nuclear testing at Livermore and the signification of the creation of the National Ignition Facility. Ramos describes the transition to his current work documenting Livermore's history, and he reflect broadly at the end of the interview on how Livermore has adapted to evolving security threats over its long history.

Transcript

Zierler:

Okay, this is David Zierler, Oral Historian for the American Institute of Physics. It is February 19, 2021. I'm delighted to be here with Thomas Ramos. Tom, it's great to see you. Thank you so much for joining me.

Ramos:

Thank you very much, David. It's a pleasure to be here, too.

Zierler:

All right, so to start, would you please tell me your title and institutional affiliation?

Ramos:

I am a physicist at the Lawrence Livermore National Laboratory. My assignment right now, I think it's officially called Special Assignment to the Principal Associate Director for Weapons and Complex Integration. How's that for a mouthful? In essence, though, it means I've been long enough at the Laboratory that nobody reports to me and I don't report to anyone. Almost ten years ago now, maybe eight years ago, I volunteered to start writing a history of the weapons program at the Laboratory for an associate director. At the time, I wanted to find a new a position and this looked promising. I enjoy history, and I was intrigued about how the Laboratory got started. The associate director I was dealing with was Bruce Goodwin. I had written a history of the X-ray laser program back in the 1980s, and I went into his office, presented him with my history, a Q-classified document, and said, "Do you know about this?" He said, "Yeah, I know about it." I said, "Have you ever read it?" "No." I said, "Well, look at it." He opened it up. I said, "I know how to write a technical history." He was impressed enough that I got the job. Originally what he wanted me to do was simply write a history of a nuclear device called the Robin because he was concerned that its history had been all muddled up, and indeed it had. As I started writing, doing my research, I kept asking myself, "Well, where did this come from? Why were they doing this?" I kept going back, and back. Next thing I know, I was writing about how the Laboratory had started in the first place. Bruce loved what I was doing, and I got myself a new job, in essence, becoming a de facto historian of the weapons program at the Laboratory. I've learned so much, and I've fallen in love with the project. There were three individuals I needed to talk to: Johnny Foster, Mike May, and Harold Brown, who were three leading characters in the early history. As a courtesy, I took a draft to Harold Brown. I found a top-secret vault for him down in La Jolla, California. Harold Brown, as you know, was President Carter’s Secretary of Defense. He spent the whole morning reading it, and I know he read it because he made marks in it. God bless him, he was eighty-eight years old at the time. When he finished, he said, "Tom, you got it right. You need to make this an unclassified history." At the Laboratory the next day, as I was walking down a hallway, the elevator doors opened and the Laboratory director stepped out, looked at me, and said, "Tom, when are you going to write that unclassified history?" I said, "Oh, God." That was eight years ago, and now I’ve finished a manuscript and it is going to be a book entitled From Berkeley to Berlin, published by Naval Institute Press. At the moment, I’m going through their publication process. Some historians have reviewed it, distinguished Cold War historians, and the manuscript received a unanimous, "Yes, this needs to be published." So, I'm happy as a lark right now.

Zierler:

Tom, it must've been hell to get this through declassification, though.

Ramos:

Yes, that's a good word. I spent about two months reviewing it myself. Then I went to the Laboratory's classification office, wonderful people. They went through it and scrubbed it. We once spent two solid days going through the draft sentence by sentence. You need to understand, some of these classification rules are complex. For example, if you said this, it was okay and if you said that, it was okay. But if you tried to associate the two, that was not okay. Edna Didwall, who used to work for me, and David Brown, the head of the Classification Office, are wonderful classification experts who helped with my manuscript. David had years and years of experience. He and Edna helped me reword sentences to avoid trouble. One thing I can share with you, I took it to the Department of Energy Classification Office, and every time I wrote the word thermonuclear or thermo-anything, they marked it top secret. I said, "What?" I once quoted Ludwig Boltzmann, who gave us the relationship between temperature and energy, which raised some classification concerns with that office. I remember the classification officer looked at me, wagging his finger at me, and said, "You physicists. You think just because it's general physics, it's okay. But once you apply it to weapons..." It was almost like saying if I said F = MA, it could be classified. So yes, it was trying. Going through my draft, my biggest challenge was, the heart of the book in my opinion, was how to explain in an unclassified way how physicists had responded to a thermonuclear challenge coming from the Soviets. That was so important, for as I bring out in the book, it affects the rest Ernest Lawrence's career at the Rad Lab. This challenge was a parallel to the way physicists felt ten years earlier when nuclear fission had been discovered in Nazi Germany. Many were upset Hitler might have an atomic bomb before the free world had one. Lawrence had the same feelings about Stalin having a hydrogen bomb before the free world had one. That became a major driver, by the way, for Livermore being founded. It's all in the book. RAND Corporation analysts took up residence at Livermore: people like Bernard Brodie, Herman Kahn, and Bill Kauffman. These were big names, as you're probably well aware, in political science at the time. They literally took up camp at Livermore, and they were working with physicists. They were trying to develop a nuclear strategy for the country to carry us into the Cold War. How do you respond to the Soviet Union having a thermonuclear weapon? To make a weapon that could credibly deter a monster like the Soviet Union, with its huge military capabilities, required special attributes for nuclear weapons, which at the time were monstrous affairs. They looked like the bomb in the Dr. Strangelove movie that was so big, Slim Pickens could ride it like a horse. These were 46,000-pound monsters. Livermore analysts realized in order to make these warheads credible and survivable, they had to be much smaller. Well, to do that was an incredible feat of physics, in my opinion. I spent months going through documents, reading progress reports of physicists of the time, and I thought, "I got it. I think I understand what it is they are doing." What they accomplished was marvelous. But then, how do I write that as an unclassified document? That was a challenge. I spent a month or two going over my draft, and the classification people helped me with words. There were certain words I could and couldn't use. Yes, that was murder and it's a long answer to your question.

Zierler:

Tom, I'm going to read the book for myself, but just for a coming attraction, I'm wondering how much you delved into the competitive relationship, for better or worse, between Livermore and Los Alamos over the years.

Ramos:

I did go into it. There's no denying it existed. As I wrote my manuscript, I made a promise to myself, because there are history books I've read that turned me off when the author expressed a personal interpretation of events. That made it more of an editorial piece than, in my opinion, straight history. I promised myself I would not do that. I was not going to naysay or say anything bad about anyone or any organization. I would let the record stand on its own and let readers draw their own conclusions. With that said, I'm well aware there was quite a tension between the two laboratories. In fact, I once picked up a top-secret document that originated at Los Alamos in the 1960s, and it had a standard disclaimer at the top, "Classified. Protect from foreign agents." This particular document had a hand-written note added, "Under no circumstances show this document to a Livermore physicist." It says a lot about the climate at the time. In my opinion, tensions originated between the laboratories due to resentment felt at Los Alamos over creating a laboratory that was a competitor. The director of Los Alamos was a man named Norris Bradbury, and it was not his nature to be competitive like that. Another factor that drove the tension, this is speculation on my part, but I bring it up in my book, was Oppenheimer. Oppenheimer was still a highly respected figure at Los Alamos. He'd hired practically the entire management at Los Alamos, and during the war, he had won the respect and camaraderie of these people. Oppenheimer went through a kind of moral turnabout after the bombings of Hiroshima and Nagasaki to where he became absolutely anti-nuke in temperament. He practically condemned the research he supervised during the war. I believe he once went before President Truman and said, "I have known evil, and I have sinned." Truman was perplexed with Oppenheimer's demeanor. Oppenheimer still held strongly to these feelings after he left Los Alamos. He was adamantly opposed to thermonuclear research, which would lead to the hydrogen bomb. The leading advocate for thermonuclear research at Los Alamos at the time was Edward Teller, which led to a tension between the two. Oppenheimer's outright opposition to thermonuclear research, I think, filtered down within the Los Alamos organization. I do know Norris Bradbury said he was not enthusiastic about pursuing the H-bomb. Within the organization, Manning who was the Deputy Director, wrote a document to the AEC that was outright opposed to thermonuclear research. President Truman finally had to order the AEC, and by implication Los Alamos, to conduct thermonuclear research to develop the Super. Bradbury followed the President's order, but without a lot of fervor. The Livermore laboratory was created specifically to promote thermonuclear research. There were grounds for tension to continue between Livermore and Los Alamos. As I bring up in my book, eighteen months or so after the laboratory was created in Livermore, there was a series of tests conducted in the Pacific where Livermore was testing a design inspired by Teller, which was not great- it failed miserably. The failure added fuel to the argument that creating a new laboratory to do thermonuclear research was a joke. At the same time, Los Alamos detonated five weapons that all surpassed expectations and did precisely what the AEC and the Pentagon wanted them to do. Now,

unfortunately, Lewis Strauss, the Chairman of the AEC at the time, ordered a news blackout. No one in the AEC could give any information about the nuclear tests to reporters. There were huge explosions going on, and newsmen were on their own to figure out what was happening. Since Edward Teller was the loudest advocate for the hydrogen bomb, newsmen thought, "Well, this must be the result of Edward Teller going off to this new laboratory," and they gave Livermore credit for all the Los Alamos successes. That did not help matters. Things went downhill from there. So yes, there was early tension, there was competitiveness. I do go into it in my book but not too much. I think it was great for the nation to have healthy competition though.

Zierler:

Tom, a question for everybody right now, but of specific import for people who work in classified environments. With remote work, with social distancing, what are the challenges, what are the precautions? How are you continuing on with your mission given what we're all dealing with?

Ramos:

Well, fortunately, at this stage, I'm mostly dedicated to the unclassified world. I've gone through classification reviews, had the whole document reviewed, so I can work on my manuscript and feel safe, as long as I stay within parameters. However, to write new articles dealing with weapons history, you're right, they'll be classified. I'll have to have access to classified computer systems back at the Laboratory or I wouldn't be able to do a thing. There was a hiatus of a couple of months where the Laboratory pretty much shut down due to a Covid-19 shutdown, but I was eventually given permission to go back to my office where I had access to classified computers: I could continue to work. The archive systems at Los Alamos and Livermore are phenomenal. I had pretty much open access to both archives as I was doing my original research. The archives were invaluable to helping me understand what was going on. In fact, just the other day, I was asked by the historian at Los Alamos if I would help write an article about the Teller-Ulam controversy with the first hydrogen bomb. If you're not aware of this, Stanislaw Ulam, was a Polish mathematician John von Neumann brought into the Manhattan Project who worked for Teller during the war. After the war, he joined Teller's group to develop a thermonuclear weapon. The work was difficult, and they were getting flustered. Ulam's calculations showed the Super would not work. Then as Ulam wrote in his autobiography, he had an idea he shared with Teller. They collaborated and things went great. I've read the classified documents of that period and it was intriguing to see exactly what Ulam was suggesting and to see what Teller was doing at the time. All of that work would've been impossible if I didn't have access to those classified archives.

Zierler:

Let's take it all the way back to the beginning. I'd like to start with your parents. Tell me a little bit about them and where they're from.

Ramos:

I was born in Brooklyn, New York. I'm second generation on my mother's side from Ireland. My maternal grandparents were both born and raised in Ireland. My grandfather, Dada, didn't make it through first grade. My grandmother, Nanny, made it through second grade. As you might imagine, they spoke with a great, rich Irish brogue. My father is first generation Portuguese. His family came from Madeira. My paternal grandfather was a fisherman who ended up in British Guiana in South America and that is where my father was born. Soon after, they emigrated to New York City. Now, my father left my family when I was three, and my mother had to work full-time to support her three children. I was the youngest. As a preschool youngster, I spent practically all my time with my grandmother. My grandfather was a manual laborer all of his life. Right up to the end of his days, he shoveled coal into hospital furnaces. That was my early upbringing in Brooklyn. I went to St. Paul's school and was taught by the good Sisters of Charity. It was in high school that I realized I was drawn to mathematics and engineering; I wanted to be an engineer. I had a Puerto Rican friend, Miguel Miranda. I don't know if he's related to the Miranda who wrote Hamilton, but Miguel said to me one day, "Tom, you ought to go to West Point." I said, "Why, Miguel?" "It's an engineering school and it’s free." I realize this sounds really naive, but it's true. Miguel helped me to write a form letter to my congressman, I went through their selection process, and I was accepted. I went off to West Point for four years and got a degree in engineering. I was not hot on physics in high school but in college I was especially attracted to modern physics. After graduation I was commissioned a combat engineer officer and stationed in Germany. I was ordered to Vietnam but through some bureaucratic mishaps, my orders had been delayed and just three days before I was to embark on an airplane, they canceled my orders to Vietnam. I ended up going to Korea instead where I commanded a combat engineer company. I had taken the trouble to write a letter to the head of the Physics Department back at West Point to tell him I was interested in going back to teach physics, and it worked. He requested me personally and I got a letter from the Pentagon that said, "You're ordered to find a university and study nuclear science." So, I fired off letters to MIT, Berkeley, and Purdue requesting admission as a graduate student, and the first letter back was from MIT and I was accepted. I sent the letter back to the Pentagon, and I soon received a correspondence that said, "You are ordered to go to Cambridge, Massachusetts, to get a degree in nuclear science." This doesn't sound real, does it? But it's all true. I got on an airplane at Kimpo Airport in Seoul, Korea, and the next thing I knew, I was in Cambridge, Massachusetts.

Zierler:

What year is this when you got to MIT?

Ramos:

I arrived in MIT in June of '75. I looked around and I remember saying to myself, "What was I thinking?" I was nervous. I hadn’t touched a textbook in six years, and I found myself having to compete with students who had been steadily cracking textbooks. If I failed it would not look good on my military record. But I jumped in, and I felt good when six months later I passed a midterm exam. The professor put the grades up, and I got the

median score, falling right smack in the middle of the class, and I felt reassured. My studies emphasized high-energy physics. I joined a group, the Laboratory for Nuclear Science, which was broken into three subgroups. Sam Ting was the leader of one, he had just won the Nobel Prize for discovering the J particle. I was with another subgroup led by Irwin Pless, where we specialized in conducting experiments with bubble chambers. Larry Rosenson, who worked with spark chambers, led the third subgroup. And I loved it. I was studying high-energy physics. We were conducting bubble chamber experiments at Fermilab and at Argonne and I wrote a thesis on one particular interaction. I had two years to get in and get out and write a thesis for a Master of Science degree, or at MIT for some reason, they call them SMs, not MSs. I got an SM in high-energy physics, and the Army ordered me back to West Point to teach physics. That was my utilization tour, as they called it.

Zierler:

Tom, how much education did the military expect you to get? What was their goal for you, and did that match your own goals in terms of your own academic interests and how good of a time you were having at MIT?

Ramos:

The orders were specific. I was to get a Master of Science degree. I had twenty-four months to do that. I had to get in there, get a Master of Science, and get back out.

Zierler:

So, going for the PhD was not up for discussion?

Ramos:

Unless I could do it in two years. The average time for a physics PhD student at MIT at the time was seven years. And in fact, all my fellow graduate students I was sharing offices with were in there for the seven years. Most of them had already been graduate students for five or six years when I popped in. And yes, I felt intimidated being around these guys. But I was simply happy to be able to study physics, even though I had to start from scratch. After a year or two I started feeling comfortable. I felt I was contributing. My thesis was an offshoot on a theory by Victor Weisskopf, who was the head of the Physics Department at MIT. He hypothesized a model called the one particle exchange, in which two particles come close together and have an interaction. It's the result of one particle, a pion as depicted in Victor's theory, being exchanged to cause the interaction. Well, my thesis was to expand Victor’s theory- to go beyond pions. The one reaction I was looking at showed that it could not have been a pion that caused the one particle exchange. Victor’s basic theory was sound; I just expanded it a bit. Then I went back to West Point for four years to teach. By that time, I'd been bitten by the physics bug: I knew I wanted to be a physicist. While my tour at West Point was ending, I applied for work as a physicist. I got two offers, one from Fermilab, probably based on my experience with the MIT group. We had been doing experiments at Fermilab, so we were known entities and I was invited out for an interview.

One of my West Point classmates happened to be assigned to Livermore and he urged me to write a job application to Livermore. Jimmy Carter was resurrecting the nuclear weapons program in the United States because of the Soviet invasion of Afghanistan and the Laboratory was hiring physicists. My classmate, and he was a former roommate, knew I was hunting for a job. He said, "Tom, fill out this form and send it to this guy at Livermore." It was like my getting advice from Miguel Miranda twelve years earlier. I filled out the form and sent it to Livermore. The next thing I knew I was invited out to Livermore. I interviewed at Fermilab and Livermore and both institutions accepted me and offered me positions. I admit, experiencing one day of Chicago weather in January made California awfully attractive.

Zierler:

Were these mostly research positions or teaching positions?

Ramos:

Well, at Fermilab, they offered me any one of three positions. But the one that was most attractive was connected with the Tevatron, which was going to be the world's most powerful accelerator. They were building an experimental laboratory they called the Meson Facility. Director Bob Wilson and the rest of the Fermilab management offered me a position to be the manager of the Meson Facility, where I'd be managing operations for universities to conduct their high-energy experiments. It was an important job, and I took it seriously. At Livermore, they offered me a position to be a physicist designer and that attracted me. At Fermilab, I would be more of a manager, managing experiments, whereas at Livermore, I'd actually be a researcher myself, and I'd be able to pursue physics. There was one caveat, I would be in the weapons program if I accepted the position at Livermore, which gave me pause. I had to think about that for a while. I didn't grow up dreaming to be a nuclear weapons designer: I dreamed of physics and discovering new things. But I felt the Livermore offer was better suited to my frame of mind at the time, and I accepted a position at Livermore. This was 1981, I showed up at Livermore and interviewed for a position in the weapons program. I went around interviewing group leaders in three divisions. John Nuckolls, George Miller, and John Immele were the division leaders, and they each offered me a position in their divisions.

Zierler:

Did you have all the clearances you needed for all three groups? Or there would be an additional clearance process?

Ramos:

I did. Based on my work in the Army when I was in Europe, I had access to NATO plans, which required me to have a top-secret clearance. In the Defense Department, what DOE calls a Q clearance is gained when you are given a top-secret clearance with CNWDI, Critical Nuclear Weapons Data Information. At West Point they were able to add CNWDI to my top-secret clearance, so when I showed up at Livermore, I had the equivalent of a Q

clearance. So yes, I was able to hear what they were doing, and I was able to engage in technical discussions with the group leaders. And there was a lot of interesting stuff going on. But there was one group leader I was especially attracted to. He had a deep, thick German accent: he had grown up in Austria. His name was George Maenchen and he is one of the greatest physicists I have had the pleasure of working with. I sat in his office and I said, "What are you guys doing?" He said, "Well, we're building a laser. We're going to pump it with a hydrogen bomb." And I remember saying, "I'm sorry, what? You're going to pump the laser with what?" He said, "Yeah," and he pulled out some papers. He had a little blackboard in his office, and he started drawing out some equations and a concept and scheme. My first thought was, "This is crazy." I thought it was so forward-looking, so challenging. And George was a very attractive person in the sense of how he presented himself and his physics. So, I went back to George Miller, the division leader, and said, "I'd like to work for Maenchen." I remember Miller saying, "Good, we need people there." Well, that turned out to be Star Wars of course. It was the X-ray laser, which became the heart and soul of Star Wars programs later on in the 1980s. But at the time I was being briefed by George, it was still something on a piece of paper kind of thing. I accepted a position to join Maenchen's group, and then I became a designer in the X-ray laser program.

Zierler:

1981, the beginning of the Reagan Administration. We're entering a new phase of the Cold War. At your level, do you feel these national developments? Are they palpable?

Ramos:

A couple of years later they were, but it was not immediately obvious. I might say that most associate SDI (Star Wars) with President Reagan. He promoted it. But I have to tell you, every single program I was aware of in SDI started in the Carter Administration. They started as reactions to the Russian invasion of Afghanistan and other reasons. So this was not a sudden lurch that came in with Reagan. This was an evolutionary process. When I wrote a history of the X-ray laser program I thought, "How did this start? What were they thinking?" Defensive weapons went back to the earliest stages of nuclear weapons– basically, as one guy told me, "As soon as somebody invented the atom bomb, somebody wanted to invent a way of destroying the atom bomb. That came immediately." In the 1960s for example, one of the biggest nuclear programs at Livermore was Spartan. There were often two teams of physicists designing the warhead in tandem in twelve-hour shifts. There was a night shift and a day shift to keep the calculations going. You might recall Spartan was supposed to track a Soviet missile in outer space and get close to it and explode to destroy the Russian warhead. But then questions came up. "Well, how do you know you destroyed it?" The next question would be, "Well, can you design the spectrum coming out of a weapon such that it would maximize its ability to destroy the Russian warhead?" They realized, "We really don't know much about the spectrum coming out of the warhead." Not much work had been done trying to understand these things when, suddenly, it became important. A physicist named Bill Grasberger was asked, "We need a code to help us try to figure out what the spectrum of the Spartan is." And Bill set to work and wrote a computer code that did that, but then the Spartan program was cancelled. The ballistic missile treaty was signed by the government and all work on Spartan ceased. But Bill held onto his code.

A physicist named George Chapline was emotionally attached to this, and he had some ideas about a new defensive weapon. He had won a fellowship and was visiting the Soviet Union, and he went to the Lebedev Institute where he sat in on a briefing from a prominent Soviet physicist named I.I. Sobelman, whose group had developed an X-ray laser. Chapline had an idea: "What if you made an X-ray laser powerful enough to destroy a Russian warhead?" With the Spartan program dying, Chapline came back with his idea, "Well, I've got something better than Spartan." What I just told you by the way is not well known. If you read a history of the X-ray laser in Wikipedia or Scientific American or what have you, you'll get a different story. But I truly believe what I just said is how it all started. It started with a bunch of very experienced weapons designers. These were very, very introverted theoretical physicists who developed a code back in the sixties and seventies to solve one problem, and then had it applied to another problem. In hindsight, it was a very natural thing to happen. I'm sorry, I can't seem to give you a short answer. But anyway, no, it was not a spurt. When it became a spurt is after development of the laser began. As you're probably aware, when you conduct an experiment, it never works perfectly. We had figured out what we thought the physics should be for the X-ray laser, but when we conducted an experiment, the diagnostic measurements showed some anomalies. "Oh, shoot. What's that about?" We thought we had a way of fixing each anomaly, we'd do another shot, and we'd make progress. Lowell Wood, who led a group of physicists, O Group, often saw the results in a very positive way. He'd say, "Eureka, this solves it." There were other physicists saying, "Well, I'm not so sure." Teller usually agreed with Lowell and said, "Yes, we got it. We now know how to make an X-ray laser." After one of our tests of the X-ray laser, Edward took a trip and met with President Reagan. And it was soon after that that Reagan announced his SDI. We were making technological advances, but some were claiming advances a little more quickly than others of us would like. Nevertheless, Reagan believed we now had the ability to destroy Russian missiles in outer space. From there, the situation catapulted and suddenly I had all sorts of newspapermen interviewing me about the weapon.

Zierler:

What was your sense of the communications and interplay between the highest levels of the Reagan Administration that was promoting SDI and the people in your world who had the technical capability of knowing whether this stuff was feasible or not? In other words, at the time, Reagan was derided for ideas that seemed fantastical, right? But I wonder, to what extent was he really prepared in announcing policies that, from a physics point of view, from Livermore's perspective, could come to fruition if the budget and political will was there?

Ramos:

There was a dichotomy. There was a political aspect, very much like we're seeing today with global climate kind of stuff. There was a political aspect to it, and there was a scientific aspect to it. And the thing is, on the political side, extremist views of SDI became the prominent voices about Star Wars. Can I use that term Star Wars? Is that derogatory?

Zierler:

No, I think that's fine.

Ramos:

I'm used to saying it that way. You had extremist promoters giving exquisite predictions of the future. Very much like the early days of artificial intelligence, how they promoted that. "Oh, God, it'll do anything." Then you had extremist detractors who were just as extreme in their negativity as the promoters were being positive. I remember joking with George Maenchen as I sat in his office, and said, "What the hell are they talking about?" One time, I even joked, "At the present moment, I think a chest X-ray would be more harmful to me than the X-ray laser." Nevertheless, the potential of the weapon was there. There were still problems from our point of view, and if you were to say, "Well, Tom, can you now deploy that weapon?" I would have responded, "No, not now." But would it eventually happen? Yes. And, in fact, to jump ahead, eventually, we actually achieved precisely what had been predicted eight years earlier, what we said would happen. What I just said is also news, because to this day, there are detractors who say that the X-ray laser program failed. I even had a discussion with Harold Brown when I was with him to go over my history of Livermore. I said, "No, we actually did achieve what we set out to do on a scientific basis." However, as I mentioned earlier, there was a dichotomy between political and scientific views of SDI. One argument was, "SDI is like going out into a rainstorm with an umbrella that has holes in it- you still get wet." That's true, that may make it a useless umbrella, but that does not make it a useless defense system. Because to be an effective deterrent, you don't need to be one hundred percent effective. If an aggressor is going to attack, he must be assured you will not retaliate. If key weapons get knocked out, even though others succeed, the aggressor cannot be assured there will be no retaliation. That could be enough to deter the aggressor. I thought the political arguments exaggerated effects. Going back to a scientific discussion, we had conducted some experiments, and we had seen some anomalies. The head of the program at the time was a man named Tom Weaver, who was Lowell Wood's deputy. He was very, very progressive. At a group meeting, as we discussed diagnostic measurements, I remember Weaver saying, "These diagnostic people don't know what they're doing. The laser is much more powerful than what they're saying." He looked at George Menschen and said, "George, would you mind leading a study to figure out what's going wrong with the diagnostics?" George kind of grumbled a little bit, but he took the assignment. George being George, he found that indeed, there was something wrong with the diagnostics. They were overshooting the signals, that is, the measurements were overly optimistic. What Weaver was hoping for, of course, was that the signals were more powerful than the diagnostic measurements were saying. George discovered it was the other way around. The diagnostics were showing a signal greater than the actual signal. I'm going to be careful not to go into details, but obviously, there was something wrong with the diagnostics and George figured out what was wrong. In a subsequent test of the laser, we used a new suite of diagnostics, which avoided the defects George had discovered. Sure enough, the signals went down precisely the way George said they would. Why wasn't the laser working the way it was calculated to work? We thought about that old laser axiom: if you pump anything hard enough it will lase, but it won't lase the way you calculate it should. That seemed to be true of the X-ray laser. Now, Los Alamos in the meantime, was

jumping on the bandwagon; they wanted their own X-ray laser program. A team of physicists came over to Livermore, and I ended up being their contact. They had conducted an experiment and they asked me to look at the results, which were anomalous. It was difficult to see if they actually produced an X-ray laser beam. You couldn't see much out of the data. I said, "Well, I think I have an idea what's going wrong. What diagnostics are you using?" When I saw their diagnostic diagram I said, "Well, I think I understand this," and I gave them a document George had written. Well, they took it back to Los Alamos and it appeared their measurements were contaminated, and worse, there wasn't any real evidence of lasing. They took that to mean that Livermore had never achieved lasing either. About six months later, I read a letter to the editor in Scientific American that said, "New evidence has arisen that Livermore never did achieve X-ray lasing." "What?" And to many readers that became the gospel truth. To this day, there are physicists who say, "Oh, the X-ray laser never worked." That's based on this one stupid letter to Scientific American, which was never verified, no one checked it. Scientific American is infamous, by the way, in that they put out stuff like that without having a peer review. But the letter to the editor was not true. So yes, there was a dichotomy between what was being reported versus what was scientifically the truth. I guess the answer to your question is, in the end, deploying the X-ray laser as a weapon would've taken longer than projected, but yes, I believe it eventually would have been capable of destroying a Russian missile.

Zierler:

Of course, these are all extraordinarily classified issues. But I wonder the extent to which the administration, DOE, the Pentagon wanted the Soviets to understand how advanced and how serious these discussions are because even the conversation, even this in concept, probably had some sort of a deterrent effect. Could you comment on that at all?

Ramos:

Yes, I can. In my opinion, yes, it did have a deterrent effect. In fact, you might want to speak with the senior scientist for SDI, Gerry Yonas, about that. He met with the Soviets, and he noted they were taking things very seriously: what we were doing at Livermore. And where I personally felt it, I'll give you an anecdote. We had one X-ray laser test, and it just so happened that on this one test, I had all of the X-ray laser experiments. I was very careful- everything had to work perfectly. I went out to witness the test, and I arrived, as was my usual practice, in Las Vegas the night before the test. And the reason I did that is I didn't want to spend a night in Mercury at the test site, where I'd end up sleeping on an old Army cot in primitive conditions. It's more comfortable to stay at a quiet hotel in Las Vegas and then call up the Lab headquarters in Mercury early in the morning to learn if the shot went off, and then go up to the test site if it did. And that's what I was doing. Next morning, I got up early; I had a Department of Energy sedan that had a two-way radio I could use to call up the test site and figure out if I needed to come in. But before that, the news came on. And the newscast said, "We have a special news bulletin. Soviet Premier Leonid Brezhnev has announced he's aware the United States is about to test the X-ray laser. If the United States refrains from testing the X-ray laser, the Soviet Union will deactivate fifty ICBM missiles." I sat there looking at the radio in the car. One, the fact we were testing an X-ray laser was classified information. Two, the fact I was in Las Vegas on my way to the test site

was classified information. No one was supposed to even know I was there. I had just heard that the Premier of the Soviet Union knew damn well we were about to test the X-ray laser. That he was willing to negotiate reducing the size of the Soviet ICBM force if we would stop testing and developing the X-ray laser was astounding. My first thought was, "Hell, we must be doing something right." Because in my mind, at that moment, I realized we were having an effect on the politics within the Soviet Union. In that sense, some of the hyperbole that was going on may have benefitted us. With Americans making these grand claims, if you're on the Soviet side, you have to believe them. "What if they're right?" And if they believe the United States was making substantial advances to destroy a Soviet ICBM, then SDI might actually be working. This was my own personal experience: Brezhnev volunteered to give up something if we didn't test the X-ray laser. I don't know if that's enough for you, but I believed something was going the right way.

Zierler:

To what extent were civilian scientists, outside advisors who had the appropriate clearances, the JASONs, for example, on site and part of the conversation from your vantage point?

Ramos:

We met several times with the JASONs. We were addressing a physics problem, and I had suggested a design for an experiment to understand this physics phenomenon we were witnessing. I had designed the experiment to amplify the effect so we could understand it better. At a meeting, the problem with the physics phenomenon was presented to the JASONs. Freeman Dyson got up and said, "I have an idea. Why don't you do this?" He went to a blackboard and drew an idea for an experiment- it was my proposed experiment. I wasn't at the meeting, but Program Director Weaver came back and said to me, "Well, congratulations. Freeman Dyson proposed the same idea as your experiment." So yes, they were aware of what we were doing, and they were contributing. Hans Bethe, who I have the utmost respect for, had done calculations, and he had written articles about why the X-ray laser couldn't work. To his credit, he was specific about what was wrong with our calculations and he volunteered to share his calculations with us at Livermore, so he was invited to come in. Peter Hagelstein was one of the theoretical physicists in the program; he had written a computer code to do many of our design calculations. I had written a computer code that post-processed Peter's code to make the output more understandable. I'll just leave it at that. Anyway, with Bethe coming in, it turned out he was claiming some of the cross sections we were using for atomic interactions were wrong. Specifically, there was one cross section he claimed was off by an order of magnitude. If you accepted his calculation, the X-ray lasing would fail. Peter came into my office and said, "Tom, we have an issue. Bethe's coming in, and I need you to run your code and show this and that." So I did. I did the calculations and we were able to demonstrate to Bethe that his calculations were wrong. Our basis for establishing a population inversion to get the gains in lasers that we were claiming to get were actually quite accurate and true. To his credit, Bethe admitted it and said, "I agree with what you have." So, I played a small role in countermanding a Hans Bethe calculation, which to this day I'm still astounded thinking about. So yes, the JASONs were coming in. Detractors like Hans Bethe were coming in. But as far as I know,

usually, we had very friendly and formative interactions. We were very interested in sharing ideas. Even a detractor like Bethe had enough honesty and integrity to admit, "Oh, yeah, you're right. I made a mistake." So I found it really nice. The other thing is, the state of atomic physics at the time, there was a lot of work going on. There were civilian groups forming to make X-ray lasers. They wanted to make X-ray lasers for medical purposes and other purposes. They had to do many of the same calculations that we had done. I remember going to a couple of physics conferences where physicists would get up and give talks. They'd have to say, "Well, we've not been able to measure these cross sections, but we think they're this, that, and the other thing." As they're talking, I thought, "Well, we've measured those things. I know what the answer is because we've done those measurements." The hang up for the civilians was energy. To set up an experiment where you have highly ionized atoms interacting to create a population inversion, which can emit a laser, you need a lot of energy. You need that for an extended amount of time. To do that in a laboratory is extremely difficult, really difficult. Whereas, of course, we had the hydrogen bomb, so the least of our problems was energy. We had a lot of energy, and we were able to do experiments. We were able to physically measure these constants, and cross sections, and things like that. It felt eerie that I was sitting there watching the civilian world trying to eke out these calculations where we already had the answers, and I wasn't allowed to say a word. That was a strange feeling.

Zierler:

By the time we get to 1986, where the summit diplomacy between Reagan and Gorbachev goes into high gear, to what extent are all of these discussions at Livermore and the theoretical and conceptual implications of SDI, from where you sat, really informing the U.S. approach to summit diplomacy?

Ramos:

Well, at that time, unfortunately, there was a big controversy at Livermore. Roy Woodruff was the Associate Director for weapons. What I'm about to say is my own interpretation. Roy got upset with what he configured to be a breach of the chain of command. He was the director for weapons research at the Laboratory, but Lowell usually went directly to Edward Teller. Edward was not in the chain of command, he was in a distinguished position at the Laboratory, but with no managerial titles. Edward could, and often did, go directly to the Laboratory director, who at the time was Roger Batzel. If Edward said something, Batzel took it seriously. In my opinion, Roy felt Lowell and his O Group were going behind his back, and he did not like that. Then, what crystallized this, if you will–remember I'd mentioned to you, we had the issue with the diagnostics being off kilter? That discovery was going on when this was happening. Our group leader at the time was Victor George, a renowned laser physicist, who joined us from the laser program at the Laboratory. Victor had a great mind, great guy. We were examining our experimental results and determining error bars. At one meeting the program leader claimed the upper edge of an error bar was the actual measurement, if you know what I'm saying. He was declaring that as the result. I remember distinctly Victor standing up and saying, "This is not physics." He was angry and he left the program. I don't think I've mentioned this episode to many people. Obviously, there was some discord going on about experimental results. As I mentioned earlier, Lowell

led a group of physicists that was called O Group. The members were all Hertz fellows. Rod Hyde was a member, as was Peter Hagelstein, and some others. Peter had some ideas for improving the laser's performance; I worked with Peter on them, and we made one of them work. But it was clear we were still having problems because the output of the laser was still not matching our calculations. Lowell and members of his O Group came up with a radically new concept design to overcome this. They put their ideas down on paper, but they needed an experimentalist to get it tested, so they had to come to our design group. After Victor left as our group leader, Steve Younger replaced him. Steve would later become head of the Defense Threat Reduction Agency. Steve walked into my office and said, "Tom, I want you to take over Lowell's idea for building a laser." I balked, "What?" I started to protest: "Wait, I don't think I"– but it didn't matter. "No, you will make this happen." I became the guy to design Lowell's new concept. While this was happening, Lowell showed his concept to Edward, after which Edward made some claims in Washington that this new concept was a game changer. I think the words he used were, "Now, a weapon the size of an executive office desk can destroy the entire ICBM force of the Soviet Union. That is, if the Soviet Union launched all of their missiles at one time, this one weapon the size of an executive office desk could destroy all of them." Meanwhile, back at the Laboratory, I was responsible for turning Lowell's concept into an experiment, and there were problems. One of the problems had to do with a physics phenomenon I mentioned earlier, when I told you Freeman Dyson had the same idea that I had for an experiment to amplify it so we could study it. That same physics phenomenon was dominating this design, and I was well-aware of the problem. I was being questioned continuously about design decisions and I found myself repeatedly having to say, "No, you can't do that. Because if you do it, I already know from experimental evidence it's not going to work." I made changes, rearranged things. Lowell came into the shop where we were constructing the experiment, along with Rod Hyde and Lowell's fiancée, Yuki, and he talked to the technicians and engineers about changing my specifications. I had to have some serious talks with the technicians about who was boss. The day to test the design arrived. This sounds naive, but at the time, I had no idea those claims for the weapon were going on in Washington. I was focused trying to make this one experiment work. We tested it, and sure enough, what I was afraid would happen, happened. It didn't perform nearly as well as those gross expectations.

Zierler:

Just to interject on that point, these gross expectations, let's just say they played out perfectly to form. What does that mean in terms of the overall strategic posture of the United States? Are we talking about, now, a world where the Soviet nuclear threat is, for all intents and purposes, neutralized? How complete is this protection?

Ramos:

If the laser had performed as well as Lowell conceived it, it would have indeed been a game changer. But it didn't. The idea of a space-based interceptor capable of destroying Soviet missiles with an X-ray beam originally goes back to George Chapline's dreams. He thought he had a better idea than Spartan, where one missile intercepted one missile. The argument that eventually killed Spartan was that it was a lot more expensive to make Spartans than cheap Soviet missiles. The argument would be, "Well, the Soviets could make us go

bankrupt by building more and more missiles, and then we'd have to outspend them ten to one to counteract it." So, the whole thing fell on economic grounds. With the X-ray laser, Chapline argued, you could put a whole bunch of lasers around one bomb, with the bomb pumping each of the lasers, and you could point each laser at a separate target. With the X-ray laser, one interceptor could destroy ten or one hundred Soviet missiles. The economic argument was reversed- it was cheaper to defend than to attack. That became the basic argument about the X-ray laser, the political argument. As I mentioned, we did achieve a workable laser, but by the time we achieved it, the Cold War was ending. If everything had worked perfectly, we might have seen a significant advantage to defending ourselves. One hiccup though: you had to have adequate warning because you had to hit these targets in outer space. I remember sitting with a couple of guys and we did some calculations about how much time was needed to get an X-ray laser up into outer space to shoot at a Soviet warhead. And I remember joking, "Hell, we'd have to have satellites over the Soviet Union. If you saw a Russian soldier double-timing towards a missile silo, you'd have to launch immediately. You'd have to react that quickly." Then we thought, "What about a submarine launched ballistic missile? That doesn't have to go halfway around the world, it could be launched off the coast. You're talking thirty seconds, not fifteen minutes anymore. You'd have less than a minute to react. There's no defense against that." While I was doing this is, I reminisced: every time we've come up with something new, when we MIRV-ed, or MARV-ed for instance, about five years later, the Soviets copied us. We ought to assume that any distinct advantage we gain is only going to last for about five years.

Zierler:

But it was a tit for tat. Every five years.

Ramos:

Correct. We'd invent something, then they would react. They would say, "Let's figure out how to do it." And the Russians are not dumb. They can do it. They had an X-ray laser program like we did. As you remember, Chapline got inspired by hearing Sobelman talk about X-ray lasers. If anything, they started before we did. So, my take was, "Even if we succeed, it's going to be a temporary blip." The question should be, "Are we better or worse off having this system in place knowing the Soviets will have it too?" And I'm not sure of the answer to that.

Zierler:

But just to play this idea out, let's say that the Soviets catch up on X-ray lasers in five years. And let's just say that the Soviets don't collapse, and we're still in the Cold War today, right? To what extent does this back and forth every five years lead to, not just a mutually assured destruction kind of deterrent effect, but a defensive effect where nuclear war is essentially impossible, even if somebody wanted to launch it? Did people think and project in those terms before it became obvious that the Soviets were falling apart?

Ramos:

I don’t know the answer to that, but in this case, I think you're going to find me to be a heretic of sorts. In a broad strategic review, vis-à-vis the United States and Soviet Union, the tit-for-tat model doesn't work very well for the initiator of innovation. Rather than continually having to innovate in order to keep ahead, it seems to me to be much better to negotiate. Get to a point where you have some form of credible deterrent. Once you’ve reached that point then negotiate with the other guy: "All right, look, we're done." However, having said that, the heresy part comes in when you’re not talking about a foe like the Soviet Union, but a foe like North Korea. If we’re dealing with an irrational power, and they build some sort of missile, ISIS gets their hands on it for instance, then the logic behind deterrence, it seems to me, changes. Classic deterrence may not be appropriate with that form of threat. Nevertheless, it would be nice to defend ourselves against an irrational Kim Jong-un or an Ayatollah. Whether a missile threat from an irrational state would require an X-ray laser, or perhaps a conventional interceptor like THAAD would work is a reasonable question to ask. A North Korea, Iran, or some other country like that, is not going to have the overwhelming power that the Soviet Union would've had. You actually might be able to effectively intercept a missile attack. In that case, defensive research is very appropriate, to me, it makes perfect sense. But to get to your question, it may not make sense against a major power. In that case, it seems to me, get to a credible deterrent status, and then start negotiating like hell. By the way, in my research, the physicists I call the upstarts, the heroes of my book, they fell into two camps. One camp, who I call the Teller camp, was very aggressive. "No, we've got to do it. If we can make a MIRV, let's make it." And then, the other camp, headed by Herb York, was like, "No, no, slow down. Stop. Let's negotiate." Mike May and Herb York would've been in the second camp saying, "It makes more sense now to negotiate." So even within the Laboratory, there was quite a dichotomy of approaches to this problem.

Zierler:

As you well know, there's a veritable cottage industry of history books that debate the extent to which Reagan's ramped-up military budget played a role in the impending collapse of the Soviets. From where you sit, what is your perspective on that- how may or may not have that affected what was happening at Livermore, both politically and technologically?

Ramos:

From my perspective, of course, the argument is not black and white, it's complicated. Did research like the X-ray laser have an effect on the Russians? I have no doubt it did, as I suggested when I gave you an anecdote. However, I think people on both sides of the argument exaggerate the effects that it had. SDI research had an effect, but did it bankrupt the Soviet Union? I don't know. While we’re on this topic, the whole issue of the rationality of nuclear deterrence is complicated. For example, when I was at the Pentagon back in '91, some in Congress were arguing for a nuclear test ban. While representing the Assistant to the Secretary of Defense, I listened to the arguments. Advocates said the United States needed a test ban in order to stop proliferation. "We need to stop countries proliferating

nuclear weapons. And the best way to do that is to stop testing ourselves." Well, the truth is, once we stopped testing, and once Bush Senior deactivated much of our tactical nuclear weapons, like nuclear artillery and theatre missiles, proliferation accelerated. It didn't stop. A testing moratorium had the opposite effect than was being argued. Local dictators were reacting to the chemistry of their region in such a way that they could see themselves being regionally powerful with nuclear weapons. They'd often proclaim, "No nuclear-armed country was ever invaded." The outcome of the cessation of testing did not match the purpose for doing it. And that fallacy continues. We see it in history books. It's such a popular argument, people just take it for granted that if we simply laid down and got rid of all our nuclear weapons, good things would happen. Historically, that's not the way it works. That's not the lesson. But no one seems to want to own up to that. Did SDI bankrupt the Soviets? I don't think so. However, it did win their attention. It may have diverted them from doing some things they may otherwise have done, and it may have had an effect on the Russian economy that might have caused Soviet leaders to be less militaristic. Would they have invaded the Ukraine sooner than they did following the Cold War? We've seen that very stringent economic sanctions that were imposed on Russia after the invasion of southeast Ukraine and the Crimea had stung Russia's economy. "Whoops. Damn, I didn't think that was going to happen." There is a deterrent effect to this. In my mind, these things all play together. Again, it is essential to have a fundamental, credible deterrent to stop some idiot like Putin from doing something awful just because he thinks he can get away with it. And I'm not feeling very comfortable with Xi Jinping in China, either. He's doing stuff we might consider irrational. This argument is an important topic in my book because the same argument was going on in the fifties. Bill Kauffman and others like him looked at the arms race and commented, "This doesn't make any sense." Curtis LeMay wanted to build thousands more bombs and thousands more bombers. It was analysts at RAND, especially Bill Kauffman, Herman Kahn, and Albert Wohlstetter, who were very prominent in this. They argued it was the quality of a deterrent force that is most important, not the quantity. Our strategy shouldn't be to match the Soviets missile for missile, bomb for bomb. We need something that's fundamentally credible, and then we can negotiate like hell. And I truly believe that.

Zierler:

Before we leave the 1980s and get to the Cold War tradition, we've been engaged in a strategic discussion, but more personally, in what ways has your role changed from your initial appointment at Livermore in 1981? Were you rising up the ranks? Did you have different responsibilities by the end of the decade than when you started?

Ramos:

I remained a designer for eight years. Personally, I had a new concept for the laser, and I was pursuing it, but I ran into opposition. I don't want to get into too much of the details, but at one point, all my experiments were removed from a test. Fortunately, I was asked to present what the results of losing my experiments would be, and I pointed out it would be a catastrophe because the diagnostics were designed primarily for my experiments and so, finally, I was given half my experiments back. We did that test and one laser broke records.

It was the most brilliant laser to be tested in history. As you can imagine, I was very proud of that, but soon thereafter, I left the program. I was sick and tired of the politics. I was not rising up in the organizational hierarchy.

Zierler:

But that kept you close to the science, and that was enjoyable for you at that juncture.

Ramos:

I loved the science. I hated the politics. The politics were getting too ugly, and it finally wore on me. I used to go running during lunch hour, and there were four or five of us that did that. Some of my better conversations were in a locker room with these guys. I remember saying to them, "I can't take the politics anymore," and one of them said, "Well, Tom, I've got a position for you." He pulled me out of the X-ray laser program and moved me over to where I was going to be interacting with the military. I was just happy to get out of that situation. I had a new position, but it wasn't the greatest. Then, George Miller, at that time an associate director for weapons, stepped into my office and said, "Tom, I'd like you to go to the Pentagon and work for an Assistant Secretary of Defense." And I agreed. I found out my job was to be the Legislative Affairs Officer for the Assistant to the Secretary of Defense for Atomic Energy. At the time he was Bob Barker, who was an old Livermore hand. We had fifteen or so committee hearings a year because his position was also connected with running the Nuclear Weapons Council, which would set the policy for nuclear weapons in the United States. I was writing everything he said to Congress. It was pretty exciting. I'd write a statement meant to be read at a Congressional committee hearing and bring it to Bob. He would edit it, changing happy to glad, that kind of stuff. Every once in a while, we'd have a strong discussion about what I'd written, but the work was pleasant. I read anything Colin Powell had to say, anything that Dick Cheney had to say, about nuclear policy to make sure we were in line with the Bush Sr. Administration. Anything they had to say, I wanted to make sure I wasn't contradicting them, and then I'd come up with my own personal take on where the future of nuclear weapons policy went. And I was happy. I was, I felt, being very successful. I got to interact with a lot of staffers on Capitol Hill-good, hardworking people. I think we all wanted to keep the country safe and do good, even though we may not exactly agree on how we did that. But they were good people. And it was at that time I became aware that Senator Nunn and Representative Gephardt were trying to set up a nuclear test ban. And I could see it coming. I was close enough to it, I said, "This is going to happen." And so, it drew my attention. I said, "What do I do in a world without testing?" Because I was enough of a nuclear weapons designer to realize you don't develop nuclear weapons without testing. They're so fickle. When we had the test ban from '58 to '61, we still developed weapons during that period, and I think forty or fifty of them turned out to have fatal flaws. And we only found that out when we went back to testing. I felt without testing we can't develop new nuclear weapons. That invites disaster. I realized a test ban was going to happen, and my attention was drawn to, "Well, now what?" And so, I switched my attention from, "How do we develop new nuclear weapons to make them more credible or to answer the Soviet Union?" to, "How do we stop rogue states from making atom bombs?" Because at this time nuclear proliferation was building up. You had the Ayatollah, you had Saddam Hussein, you had Assad in Syria. In Libya you had Ghaddafi. They were all

starting up nuclear weapons programs. And I turned my attention to, "I wonder if we can stop these guys from doing that." When I got back to Livermore, I was reassigned to the analysis division, and I took over a group. I had an idea and I drew in members of the group who were helping to rebuild the nuclear weapons complex of the United States. Their emphasis was to make it cleaner, less radioactive, less radioactive waste. It was a big deal back in those days. We had too much radioactive waste being created in the old sites. How do we design a new complex that's more efficient? My guys were experts in how to build a national complex to make nuclear weapons and I said, "Okay, I got that. But I don't want to do that. How do I stop the Ayatollah from building an atom bomb?" And we started up a program to answer that question. We started off by looking at North Korea, and we started to analyze that. If we wanted to stop them, how do we do that? And I said, "I don't want to destroy Korea, and I don't want to destroy Japan by targeting a big reactor at their site. It's taboo, we do not bomb the reactor. So, can I still stop them without causing any kind of collateral damage? That's the trick. And how do you do that?" I had some real experts who could analyze exactly what was going on in each of the buildings at their site. We had satellite imagery and we started to figure it out. I took our analysis to Washington and met with the President's envoy to North Korea and showed him what we were doing, and his eyes lit up. He wanted to know, "What if we did this? What would happen if we did that?" I knew I had a good idea. And then, I took it to the Pentagon, went to the Joint Staff, and it took a while, but it finally got accepted. And my big breakthrough was in the Kosovo War; we had analyzed the chemical weapons program of the former Yugoslavia. One of the guys in my group had built half the chemical plants in that part of the world. So we knew how they were doing stuff. And I said, "Well, we know how to stop the chemical weapons program. We could stop it if we wanted to." And I put this stuff out on a classified network, and we did one particular site. And I color-coded buildings at the site. I said, "Green is what you want to go after. Red means bad collateral damage. You don't want to touch it." In this one site there was one building, it was red of course, that had seventy metric tons of liquid mercury in it. They were using liquid mercury for a chemical reaction. And our analysis said, "Do not drop a bomb on this." Well, it turns out the Air Force went and bombed the facility, including that building. The mercury spilled out, went into the Danube River, and contaminated the Danube. And it made world news. It was a big black eye for the Air Force. My program was called the Counterproliferation Analysis and Planning System (CAPS), and someone said to the Air Force planners, "Why didn't you guys look at CAPS?" And they responded, "What's CAPS?" That's when the Air Force got interested in our work. "Holy God, I wish we had known this." And then, we married up with the Air Force and my program took off. I started with a $200,000 grant and by the time I left the program, it was a $46 million a year enterprise. Secretary of Defense Cohen called it the best counterproliferation program in the Defense Department. I was really proud of my guys. I had 120 people working, and I had some outstanding chemical engineers. For instance, I had hired the lead chemical engineer for Chevron as he was retiring. He became one of my analysts. I was hiring people who were retiring. I hired the former head of the Trident program, the guy who designed the third stage of the Trident missile. He became one of my missile experts. I had world-class guys, and we were analyzing threats around the world: "How do we stop WMDs without killing people?" I'm happy to relate that twice our analysis went up to the President of the United States, once to Clinton and once to Bush, and in both

cases, they canceled an operation that was under planning. We had said, "No, no, no. You don't want to do that because you're going to kill people if you do that." I was really proud of the fact that they were using our program, and it was becoming very useful.

Zierler:

And just to orient ourselves in the narrative, it's obvious when you're talking about dealing with a possible test ban, and now you're becoming increasingly focused on terrorist actors, and rogue states, and things like this, this is very much a pivot to a post-Cold War world. So before we more fully develop that, the end of the Cold War obviously is not a dramatic thing. It doesn't happen in a day. As we say, from the fall of the Berlin Wall all the way to the end of the Soviet Union. So there was plenty of lead time to detect that the Soviet threat was going to be coming to an end. At least the USSR. What, from your perspective, was Livermore's overall posture to these developments, both in Eastern Europe and in Moscow, during the '89-'91 period?

Ramos:

I'm trying to be careful; I don't want to give a false impression. There was a bit of chaos: bit of a rudderless boat. The big thing that struck me during that time, to be more parochial, was nuclear testing was ending. And I think a lot of the management at Livermore was trying to figure out, "What do we do now?" And Livermore's budget was imploding. Many of the senior people had been around so long, that was their whole world for decades. And in my opinion, there was a little bit of trouble adjusting to a new world where the old tried and true ways of doing things were no longer relevant. Plus, it was interfering with our traditional role, which frankly, was to be usurpers to the established norms. A distinguished political scientist came to the Lab seven, eight years ago, and he was giving a lecture on the history of the Cold War. And he said, "There were periods when the political situation was fairly stable but there were four instances where the stable status of nuclear deterrence got rattled a bit." And he brought up MIRV-ing, MARV-ing, and SDI might've been another one, in which it kept the Soviet Union having to react to us. I raised my hand and said, "Are you aware that each of those four things you mentioned that were significant all started here at Livermore? Every one of those things were the result of something happening at Livermore." And he said, "I had no idea." I said, "It's true." And I'll give Edward Teller a lot of credit, I think Edward was part of this stirring the pot. It fell in, I think, with the legacy we got from Lawrence, originally, with his being concerned about our national security. But anyway, there was this kind of dynamic at Livermore that I noticed. I had been a visiting scientist at Los Alamos, and I got to see the culture there quite a bit while I was still in the Army. When I got to Livermore, it was a distinctly different environment. It really was a different environment where upsetting the status quo with new and perhaps crazy ideas was welcomed. Now, in that period in '91, that world seemed to have vaporized rather quickly. I think some leaders had trouble adjusting to that turn of events. That's when we adopted stockpile stewardship. That became the new thing. But I could tell you this, I've had wonderful conversations with Johnny Foster, and Johnny and I both agree that Vic Reis's stockpile stewardship idea, it's good, but it's good for about five years. After that, we're back into the world of, "Are you going to use a computer to verify this is all going to work?" That can't go on forever. It's a good idea and we could certainly have a million

things to learn with models that we have, and understanding thermonuclear environments, and understanding stuff. But without testing, your reliability goes down. It asymptotically decays regardless of computer modeling. Then we started seeing a hemorrhaging of talent. In my opinion, I'm trying to be careful, the Laboratory started hemorrhaging.

Zierler:

Hemorrhaging in the sense that people could see that Livermore's strategic utility during the Cold War was existentially threatened and people wanted to get out before there were massive budgetary cuts? Is that what you mean?

Ramos:

No, I don't mean that. What I mean is, when I got hired, they were after excellent people and not looking to recruit for one particular program. "Show me your credentials. What have you done? How good are you? Do you understand this stuff?" It was like that. And, "We have a mission to accomplish. We're going to do this." In the nineties, they're trying to recruit top notch people who are emotionally charged, who want to make a difference in the world. Prospective recruits arrive and say, "Well, what's your mission?" And frankly, the Laboratory really didn't have a well-defined mission. If you're trying to recruit the top of the line, these are people who want to make a difference. And believe it or not, accomplishing something for the security of the country, getting to see something get on a shelf and do something, was very satisfying. We were getting top notch people dedicated to doing that. But now, that mission went away. Plus, top notch people within the organization, I think, began to say, "What's my purpose? To jockey a computer?" There's nothing going into a national arsenal. There's nothing tangible going on that you could say, "This is what I did for the security of our country," for example. That mission evaporated. And nothing had come in to fill the void. That was going to take a while. There were many other programs coming up, but there was no big strategic plan, I think, for the Lab to redefine itself in a new environment.

Zierler:

But that may have been imposed externally. What were the budgetary implications coming from the Pentagon, the new administration, and the Department of Energy?

Ramos:

Sure, you have constraints. But let me bring up the calculus of variations. You're going from A to B. What's the optimal way of going from A to B with constraints? Well, new constraints were put in, but we still need to go from A to B in an optimal way. We need a mission to accomplish. For example, one idea I had was, "We have a phenomenal amount of experience with high explosives and how to make them do the craziest things." I thought, "Is there something we could actually make, a super boutique level conventional weapon that could do something that never had been done before? Could we go after targets that, before, used to simply be nuclear targets because that was the only thing with enough power, but now, with the exquisite accuracies of these systems, could we make a

conventional weapon that could accomplish something that was considered impossible? And would that be useful? Why don't we at least get something in there? We're still a national security laboratory. We, as a nation, have decided to quit nuclear testing, it's not going to be easy to develop new nuclear weapons, so what do we do?" Now, before I go too fast, it's possible nuclear weapons will need to have characteristics that may not have been appropriate during the Cold War but may now be appropriate for a post-Cold War world, in which nuclear threats may be coming from China, North Korea, or other places. We're the thinkers. Back in the 1950s, we had the Bernard Brodies, we had the Bill Kauffmans, we had the Herman Kahns grasping a new world of nuclear weapons and how you make sense out of that. Wohlstetter said, "This is not an easy problem. This is incredibly difficult." And what I think is missing now is dealing with these changed circumstances. The constraints of your calculus of variations equation just changed dramatically. In the fifties we had all that spirit, these people coming together, big thinkers working with physicists to solve a serious national security problem. Because remember, we were developing a weapon we could never use. We can't use it. And that's a conundrum. It's a huge conundrum. And people like Mike May, Johnny Foster, and Harold Brown tackled it. The climax of my book, by the way, is a nuclear crisis in which we forced Khrushchev to step back. And Kennedy admitted that Livermore's contributions at that crisis were crucial to averting a nuclear war. This was not the Cuban Missile Crisis, this was much more serious, and he flew out to Berkeley to personally thank those guys for averting a nuclear war. Back to your question, one might ask, "So what do we do now? Where's that energy that we had in the fifties? We need it in the nineties." I think the explosive energy I felt at the Lab in the 1980s, where I thought, "I have a purpose," was missing in the 1990s. I'm still at the Lab, and I still have my two cents to offer. What do we do in this new world? We can't just sit back on our laurels. We have a rising China. We have a Russia with this guy Putin running it, and he's bragging about his tactical nuclear weapons. What if he uses a tactical nuclear weapon on Lithuania?

Zierler:

On that point, right after the collapse of the Soviet Union, there were many serious strategic thinkers, not peaceniks, not people who had naive expectations about the future of international security, but serious people who contemplated the idea that with the end of the Cold War would mean the end of international conflict on a large scale. And yet, hearing you talk about the pivot to understanding the threats from North Korea, and Iran, and rogue states, and terrorist cells, and things like that, I'm curious if this pivot more broadly represented some nimbleness on the part of Livermore, who recognized that it may have been born out of the Cold War, but its overall contributions to American security were not limited to a Cold War framework. Were those the kinds of discussions that were happening at a strategic level within Livermore?

Ramos:

If there were, I was not part of it. I didn't see that.

Zierler:

So then, what explains- because these are actual decisions that need to be made bureaucratically, administratively, where this pivot is actually operationalized, and Livermore ensures that it remains relevant to U.S. national security policy?

Ramos:

I think we went into a reactive mode. Frankly, we allowed events to dictate the course we were going to take. So early on, in the nineties, it's stockpile stewardship. But then, with the hemorrhaging, morale started going down. People who were in the weapons program started looking for other parts of the Laboratory to do other things: nanotechnology and additive manufacturing for instance. There were some great things that popped out of this. But these were people who were previously working on a weapons program who then diverted their efforts to other things and came up with some good inventions. But when they achieved success, several left the Laboratory. Because they had patents now, and they were becoming experts in these fields that are lucrative, additive manufacturing being one. That was kind of the state of things. Everything was kind of reactive. In my opinion, morale was sagging. Then 9/11 hit. And for a while, within the upper echelons of the Laboratory, there was a stream of thought that, "We'll become the Homeland Security laboratory. We're going to do that." I was having problems with it because I realized, "We don't stand a chance. We're not going to survive very well in that kind of world," which turned out to be true. For a couple of years, the Lab emphasized scientists who were inventing detector systems. They could detect an atom bomb being smuggled into the country or a biological agent secreted into a subway system. The Laboratory began to reorganize toward that end, and budgets began to emphasize that kind of research. To me, there was a further deterioration of what was originally our core mission. My issue, I think, which is what you were alluding to earlier, was unfortunately the nuclear threat in the world had not gone away-it was evolving. And I guess my argument was, we should've been evolving with it. My research with my book reminded me that events in the nineties were similar to events after World War II, when the Soviet Union emerged as a nuclear power. In August of '49 they detonated an atom bomb and in my mind that started the Cold War. Once they had the atom bomb, we faced an aggressive nuclear armed Soviet Union. Lawrence was just as agitated in 1949 as he had been in 1939 with the discovery of nuclear fission in a chemistry laboratory in Berlin. He saw Vannevar Bush and said, "We need to start an atomic program. Hitler's going to have an atom bomb." And ten years later he said, "Stalin's going to have a hydrogen bomb and if we're not careful, he'll have it alone." The Soviets were developing a thermonuclear weapon, they had already hired Sakharov. A physicist named Tamm started up a thermonuclear program in the Soviet Union almost immediately. In the 1950s, a group of world class minds came together at Livermore. Herman Kahn, Bernard Brodie, Albert Wohlstetter. I like to joke, and I told this to the Vice President at RAND, "You know, RAND Corporation grew up at Livermore." I can show you one paper after another was written in the fifties in which physicists and political scientists were linking their thoughts. There was a lot of strategic political thinking going on, with the political scientists asking, "Can this be technically feasible?" It was that miracle in the fifties that got us to face the Berlin Crisis of 1961. In my mind, as I'm seeing with a resurgent Russia and resurgent China, we need that kind of thinking going on again. How do we respond? It's not clear to me but it’s not by

building more bombs. You have to be clever because Putin doesn't strike me as the most rational individual on the planet. You want to keep him within limits and the best way to do that is with a deterrent that even he would recognize, "Uh-oh, that's not a good idea to do that."

Zierler:

As we get to the late 1990s, I wonder if you can comment on the impact of the Wen Ho Lee case on security operations at Livermore.

Ramos:

I remember going into a Laboratory cafeteria and there was an employee sitting at a little table at the entrance handing out leaflets that proclaimed Wen Ho Lee was being persecuted. Regardless, he was in my opinion extremely dangerous. After the discovery of Lee's releasing information to the Communist Chinese government, the Chinese started a series of nuclear tests. This was happening while I was still at the Pentagon. The attributes associated with those tests were very similar to data Wen Ho Lee was accused of exposing. I think Lee's actions accelerated China's nuclear program. At the time there was kind of worldwide nuclear test ban. To keep that kind of equilibrium, if China developed a thermonuclear weapon from scratch instead of stealing the information, it would have taken a long time. Without espionage, they'd be going out trying to test where they wouldn't know exactly what they were doing. It would take them years of research. That would be a deterrent to keep them from doing that not only because it was difficult, but they'd be antagonizing enemies. On the other hand, if they have the answers, if they already know, "This is going to work," and then just need to verify what they think they already know, they can do that in a matter of two, three, four tests, and they're done. And that's exactly what they did. I don't know for a fact if that gave Xi Jinping the audacity that he's now showing, building up a large Chinese stockpile. They may go out on military adventures. Would he have been able to do that without Wen Ho Lee? I'm not sure.

Zierler:

On a very related question, what was Livermore's initial reaction to the creation of the NNSA, and how has that played out long term?

Ramos:

Speaking for myself, my first reaction was, "Thank God." Because Clinton's Secretary of Energy was a character. She came out to Livermore after she was appointed, appeared at the main auditorium, I was sitting there, and she said, "For every successful career, it usually involves three or four changes in your career. You people need to be thinking that now."

Zierler:

Hazel O'Leary.

Ramos:

Thank you, Hazel O'Leary. O'Leary basically was telling us, "Hey, I'm going to shut you people down." But then she left the Department under a cloud. I saw her once when I was in Omaha on travel. She was going to a conference at STRATCOM and she came with a coterie of thirty or so youngsters. I thought she was a teacher bringing high school or college kids with her as they stood around the baggage carousel. And I thought, "Wait a minute, that's the Secretary of Energy." She brought these thirty to thirty-five members of her staff, and when the commander of STRATCOM gave her the schedule for the meeting she responded, "Oh, no, don't start before ten o'clock because I always start each day with a body massage." She expected him to provide a masseuse to her room. He had given her this big house to live in, and she insisted she would have a body massage before the conference began. It was horrible. Things had gotten so bad about the well-being of our nuclear weapons within Department of Energy that Congress felt they needed to create another agency to act as a sort of filter. My first reaction was, "Thank God, things are going to hell in a hand basket." With the creation of NNSA, events happened that were already happening within the Department of Energy. There was more and more micromanagement of the Lab. When I arrived at the Laboratory in 1981, there were maybe six Department of Energy people here, and I recall they occupied an old wooden building. The main DOE offices were back in Oakland and San Francisco. Now there was an entire NNSA office building at Livermore. And the number of bureaucrats who have direct jurisdiction over experiments at the Laboratory is exploding by orders of magnitude. Even highly technical areas, like a geological experiment, something for geophysics, needed to go through a mountain of paperwork to have it approved. And the approval authority was some college graduate with a political science degree. I'm exaggerating, but not by much. It's stifling. A lot of that free energy at the Laboratory, where physicists could shoot out new ideas like "Let's make an X-ray laser," or "Let's do the human genome" was diminishing. Decisions like that used to be made at Livermore, like when Mort Mendelsohn, who had been recruited from the University of Pennsylvania, started a human genome project. He didn't have to ask anyone, he just did it. That type of freedom has diminished. The bureaucracy is bigger.

Zierler:

To return to the more personal question, for the nineties, what was your role at this point? From the end of the Cold War to the 9/11 era, what were you doing along those periods in terms of your rise at Livermore?

Ramos:

Associate director George Miller was going to do away with the analysis division because we were no longer testing. With news of that, the division leader quit in a huff. I got a call from John Nuckolls, the Director, asking me to take over the division on an interim basis-in addition to my group, I took responsibility for two other groups. Immediately after that, Miller, who was the Associate Director for Defense Sciences, called me in. At that time, the division's budget came entirely from the nuclear weapons program, because we analyzed how nuclear weapon systems fit into the national security structure. He said, "Tom, your budget from me is going to go to zero in four years." I said, "Okay, George," and called a

meeting with my group leaders and told them the story. I said, "Okay, we have to rethink what our mission is and where our funding will come from. We can no longer just sit back and do only nuclear weapons analysis. It's going away." That's when I created a counterproliferation program within my group, in which the money would have to come from Washington, no longer from the nuclear weapons program, and my inclination was to work with the Pentagon. I started with a $200,000 grant from a good guy at the Department of Energy, Ed Fei. Growing a program from scratch with no established sponsor was very difficult. At one point I called a group meeting and told all my group members they had to find jobs around the Laboratory. I said, "Don't quit, I want you to stay in my group. But I need you to get off my budget for a while and go find jobs around the Laboratory. Somehow, I'm going to get a program started, and I'll call you back." And they all did it-they were good. For the next year, every other week, I was in Washington trying to drum up support. Just giving lectures, giving presentations to anyone who would listen, showing them an analysis we did, and slowly, I was able to build up interest. I had some big breaks. Bob Joseph, who is a professional friend I had met at the Pentagon, had become the head of the Counterproliferation Center at the National Defense University. He was starting up this counterproliferation effort and he came out to the Lab and saw what I was doing. Later, Bob became a member of the National Security Council. He went back to Washington impressed with my ideas and how I was planning to implement them. He spoke about what he had seen to an influential friend, Chris Williams, who happened to be the chief of staff of the House Armed Services Committee.

Zierler:

Who was driving that? Who was really supporting this work in Congress?

Ramos:

Chris Williams was a big one. I don't know if you know Chris. He was Rumsfeld's choice to be the Undersecretary of Defense (Policy). He's a hell of an intellect. One day Chris called me, "Hey, I was talking with Bob Joseph. You're doing something interesting out there at Livermore and I'd like you to come to Washington and tell me about it." So I did. And he said, "Well, this is great. How much money do you need?" I was not used to that kind of talk. I'm not used to kind of world at all. I said, "Well, I'll have to think about that," and later I gave him a plan. I think it was for a program that would need $2.8 million. "If I had $2.8 million, I could hire a guy to do this, I could hire a guy to do that, and I could analyze this many countries." "All right." Next thing I know, funding was given to STRATCOM, they hired me, and I did what I promised I would do. We started doing more powerful analyses as I brought more people into my program. I was careful only to hire top notch professionals who knew what they were doing. I mentioned I had hired the senior chemical engineer from Chevron Corporation. These were world-class professionals I was bringing into my program. And we started doing some good things. And, of course, proliferation was exploding. My relationship with Chris Williams stayed close over the next few years. He is a great friend and we still communicate. By the way, he endorsed my book, he's on the endorsement page. Wonderful man. Bob Joseph remained a fan of the program too as well as several other prominent members of the Defense Department. Ironically, I'll tell you, the members of Congress who ended up supporting the program the most were liberal

Democrats, if you can believe it. Actually, it made perfect sense. When Martin Sabo was the head of a subcommittee in House Appropriations, I think he had a defense subcommittee, he became a big fan of what we were doing. He was very prescient; he thought, "Looks like you guys are actually making some common sense out of this." And it turned out to be true because the two times that our analyses reached the President, it caused the President to stop a planned operation because the military planning had not considered all the consequences of a military engagement that my program brought out. It was a really important program that took years to properly mature. I eventually got it POM-ed. That's an acronym: POM is a program of merit, I think. But I'll tell you this, if a program is POM-ed by the Pentagon, it's part of something called their five-year development plan, which means the program is a permanent part of the defense budget. And my program became the only POM-ed defense program in the entire national laboratory system. That's how much respect we were getting from the Pentagon. My program was a Defense Department program, not a Department of Energy program. And it brought me in contact with influential people. I remember one meeting, it was with a member of the National Security Staff, and I looked out his window and eyed the Rose Garden next to the Oval Office. It drove home to me how important our work was. I was very proud of that, but it came with a price. It aged me having to deal with Washington/Livermore politics. The more my budget went up, the more corporations, what we call beltway bandits, were catching on to us: "Hey, wait a minute. What's going on?" And the beltway bandits coveted our budget and they put on more pressure to take over the program, which thankfully, was not easy for them. As I mentioned, I took my program's budget from $200,000 all the way up to $46 million per year. But then I had to leave the program and it's since dropped down again. In the 1990s, my entire life was taken up with growing this program from nothing into one of the biggest programs in the Laboratory. I even had Ed Moses, the head of NIF, call me into his office one day, "Tom, how do I do this?" I said, "Ed, what are you talking about?" I had an incredibly successful effort and Ed wanted to accomplish the same thing with his NIF program. The reason for my success was I had the right professionals and it was an appropriate mission for those days. We started as proliferation was growing, terrorist organizations were getting bigger and bolder, and weapons of mass destruction were being recognized as the biggest threat. What do you do when a hostile country is covertly producing weapons of mass destruction? How do we handle that? And that was very, very appropriate for the 1990s, and that's why the program did so well. And that's why I'm trying to tell you that for now, in February 2021, we need to be rethinking this because we have a new set of problems. We need to rethink, "How do we address the threats arising now?" We were engaged in the longest war in the history of America, during which my CAPS program was constantly part of the national security posture of the nation. It's one way the Laboratory stayed involved with national security. The rest of the Laboratory was pretty irrelevant to the wars going on in Iraq and Afghanistan. The country was caught in its biggest national security crisis of the 1990s and for the most part, the Lab was irrelevant to it. I was very proud of our contributions.

Zierler:

In what ways was your program responsive to Livermore's strategic mission post-Cold War?

Ramos:

From my point of view, it was not. My biggest problems, frankly, came from inside the Lab. My program was considered orthogonal, or even threatening, to the other programs in the Lab, yet organizations still appreciated that it was a big program, and the Defense Department seemed to love it, and that was as far as it went. As I mentioned earlier, there was a big movement towards detectors. "Let's make detectors" for Homeland Security type missions. And here I am running a Defense Department program. We were not part of the day-to-day activities going on at the Lab.

Zierler:

What were some of the developments that you did not see coming along the way?

Ramos:

The National Ignition Facility (NIF) was coming to fruition while I was at the Pentagon, so I got to see it from a Washington perspective. And the way I saw it, Sam Nunn was trying to convince many conservative senators to join up with a test ban. To make his argument, he had to convince them we would not be weakening our national security. He argued we could create a facility at a national laboratory that could mimic the environment of a thermonuclear weapon, and through that facility, our knowledge of thermonuclear weapons would stay current, and we would not suffer. It would replace nuclear testing. And this magical facility was called the National Ignition Facility. It was going to reside at Livermore, which was natural since Livermore had one of the most prestigious laser programs in the world. NIF was going to become a replacement for nuclear testing. That was all fine, but then the director of NIF, Mike Campbell, was relieved of his position because he was accused of running over his budget–by the way, Mike is an outstanding physicist and manager. When I tell you about my being in Washington, giving briefings at the Pentagon, or responding to calls from the legislative staff of SOCOM, more often than not, I'd see Mike in Washington as well, trying to keep NIF alive. Mike's success stoked the ire of Bill Richardson, who had become Secretary of Energy; he had been a Congressman from New Mexico. One of many things I witnessed in my dealings in Washington, especially with Senator Domenici, was that he tended to support projects in New Mexico, especially at Los Alamos and Sandia: they were favored sons. But it could be was worse than that; for instance, when it was suggested that Congressional funding for a project could be shared among all the national laboratories, Domenici would almost surely squelch the initiative. My experience was if he saw the word Livermore on a Congressional initiative, he killed the whole thing. Rather than give funding to Livermore, even if it was being shared with Los Alamos and Sandia, he'd kill it. That kind of attitude might be found pretty much throughout the New Mexico Congressional delegation, including with Richardson. Now he'd become Secretary of Energy. Here is NIF, a premiere multi-billion-dollar facility being built in Livermore. And again, this is just me, but I think Richardson was not happy about that whole thing. He made accusations of mismanagement at Livermore about spending exceeding the budget. It was complicated, but in my opinion, it was not totally Livermore's fault. But Richardson made this a cause to kill the project. This was happening at about the same time, by the way, when Domenici was talking about changing the University of

California contract signed during the Manhattan Project. Los Alamos should be given a contract with the University of New Mexico instead. There followed accusations of mismanagement at the weapons laboratories in order to discredit the University of California. This attack on NIF, I think, was part and parcel of that legacy. Anyway, Campbell got relieved and Ed Moses came in. Ed's an engineer, he's not a physicist. He did a superb job getting NIF built. However, part of his character had to do with his need to control his projects totally. Everything had to be done, even the physics, under his control. The design teams, the teams that were designing targets for NIF, had to be part of the NIF organization even though the professionals with the expertise to do that work were nuclear weapons designers. They'd been doing that for half a century. Code development was being run by laser physicists, and that was not necessarily their world. In essence, Ed kind of hijacked the NIF program to make it into a fusion reactor, not a laboratory to recreate thermonuclear environments. He called his initiative the Life Program. That I did not see coming. Of course, without nuclear physicists leading the research, it took a while for NIF to achieve ignition. It almost brings back memories of Star Wars with its great early promises. I attended briefings about the Life Program: "We're now going to have fusion energy for the world." It was the same kind of phenomenon-super positive thinking. In the meantime, though, the nuclear weapons program was separated from much of NIF operations. So, the reason NIF was built, in my opinion, did not play center stage. I did not see that coming.

Zierler:

How did all of this affect your relationship with the program as it was reaching a more mature stage?

Ramos:

Well, I think I mentioned to you, Ed called me into his office one time asking me for guidance on how to secure funding for a big program. It was a friendly meeting. The Laboratory started taxing its programs more heavily, some of it to help the NIF project. And I've forgotten how much funding I ended up sending off to NIF, which upset me at the time. I remember thinking, "I think it's time NIF got off the wagon and started to push it with the rest of us." So in that sense, it was not all that friendly. But it never got bad. I admire Ed. I think he's a hell of a talented guy.

Zierler:

Tom, what came next for you?

Ramos:

Then, the CAPS program got too successful. And it goes back to what I mentioned to you, where I was pretty much orthogonal to things going on at the Laboratory. I felt a kind of jealously from a few other Laboratory programs, most likely because my budget had risen to $46 million at a time when the Department of Energy budget was going down. A lot of programs were suffering at the same time I was expanding because I had DoD funding and

was immune to DOE spending. I remember seeing Lowell Wood at the Rayburn Building about this time in Washington, and I commiserated to him, "Lowell, everyone's trying to kill me back at the Lab." He looked at me and said, "Tom, the more successful you are, the more enemies you're going to make." And he was so right. Eventually it got to a point where I was ousted from the program. Unfortunately, there wasn't much recognition of the program's accomplishments or that it was the premiere Defense Department program for counterproliferation. I needed to avoid having no position, of remaining an employee between assignments, an EBA, which meant after about a year I'd be asked to leave the Lab. Frankly, I felt like leaving because I was growing tired of politics and I contemplated retiring. I'd had a good run with a career at the Lab. I had a really great time with the X-ray laser, and I had a wonderful time with my CAPS program; "It's time to leave. I'm in my sixties." That was when I heard Bruce Goodwin, the Principal Associate Director for Nuclear Weapons, wanted someone to write a history of the Robin, which is an atom bomb Laboratory designers had invented back in the 1950s. I thought to myself, "Shoot, I can do that. That would get me out of this situation I find myself in, and I'd get to do something interesting." I went into Bruce's office and showed him my history of the X-ray laser. I'm a good talker, as you can tell, so I talked myself into a position. He gave me a temporary assignment to write a history of the Robin. I started digging and quickly discovered I had no idea how we came about developing the Robin. I kept digging and digging. After a few weeks, I went back to Bruce, and said, "Well, this is going to be deep, Bruce. If it's okay with you, instead of me giving you just a simple history of the Robin, I'm going to have to tell you how and why the Laboratory was created, because unless you know that you won't understand what I'm talking about." He agreed. I started writing about the early history of the Lab, and Bruce said, "Tom, this is great. I want you to give lectures to the weapons community at the Lab. And I started to do that and was gratified when the physicists, scientists, and engineers serving in the weapons program got excited about my research. This was brand new stuff for everyone. The next thing I know, my portfolio grew and grew until I was researching a comprehensive history of the nuclear weapons program at Livermore. The result was a classified document completed in 2013. As I mentioned to you already, Harold Brown saw it and said, "You need to make this an unclassified history." I reinvented myself as a historian and began my third career at Livermore: from X-ray laser designer, to CAPS program leader, to nuclear weapons historian at the Lab. For the last eight to ten years, I've pretty much been writing history. I don't feel guilty because since CAPS is a POM-ed program for the Defense Department, it continues to bring funding to the Lab. The Laboratory uses my history work well. I give lots and lots of lectures to newly hired engineers and physicists, where I give the message, "This is our legacy. This is where we came from. This is why you're doing what you're doing." I also give lectures to federal employees from the FBI, Homeland Security, Department of Defense, Department of Energy, and State Department. They're coming in to get an orientation about nuclear weapons. My basic message is: "This is what we did in the 1950s." The State Department guys are really interested in how political scientists at Livermore shaped a lot of our strategy for the Cold War: "This is not about a bunch of physicists inventing bombs. This was a highly coordinated effort between sophisticated political scientists with sophisticated physicists, and they came up with a strategy for the Cold War, and it worked. That's your lesson for today."

Zierler:

We're thirty years out from the end of the Cold War, twenty years out from 9/11. If you could reflect on the impact of both of events on Livermore, what was bigger?

Ramos:

In a way, they were equal. One in the sense of Livermore pulling in its horns. At the end of the Cold War, there was a listlessness, not knowing where we're going, loss of mission; professionals were pulling in their horns and wandering around. It made a big impact on where the Lab was going to go. As I've said a couple of times, in my opinion, it caused some hemorrhaging where we were losing people and not gaining people. And then 9/11 kind of reinvigorated a new mission to fight terrorism. And that's when a lot of programs kicked off: non-weapons programs. Both events were inflection points that caused a change of emphasis. I don't know which one was bigger though.

Zierler:

Another broad historical question. Given all of the presidential administrations that have come and gone since your tenure at Livermore, what have been among the most effective in terms of allowing Livermore to do what it does best?

Ramos:

I look back to the Reagan years, when we pretty much had carte blanche. And I don't think the money was wasted. We accomplished huge things. So definitely, the Reagan years, in my tenure, were great. Reverting back to my book, they were great because of one of Lawrence's legacies: he introduced the concept called matrix management. When I got here, matrix management was very much alive, and it was wonderful. Physicists often met with chemists, mechanical engineers, electrical engineers, and code developers who were members of the same program. We'd have meetings, and each of us had something to say. And it was a nice, innovative type of environment. Over the years, that ethic has kind of eroded away, and we've evolved to more of a standard managerial kind of thing where instead of scientists and engineers working directly, we have to go through different layers of management. If you want to talk to someone inside your program but outside your group, you've got to first talk to their group leader. All too often the group leader will say, "Well, what do you want to know? I'll handle that." No more of that freewheeling stuff. When I got here communication among members of a program was freewheeling, and crazy ideas were just flowing about. At first, it was a shock to me because I was used to a military type of environment. I even watched arguments where George Maenchen would have a disagreement with a program leader, and I'd expect to see George get fired. But no, that was the spirit of this place where professionals had a voice, they were smart, and they felt free to contribute. We had a computer culture at Livermore. It was a rich computer culture, which based on my research, I now realize came from John von Neumann himself. Von Neumann was here day after day encouraging scientists to write computer codes. In the early 1950s during the first two years of the laboratory, von Neumann was a daily participant with the weapons program at the Laboratory. I became aware of that while interviewing Mike May

and John Nuckolls, when I asked, "Well, what happened when you arrived here at the Lab?" And the first thing they said was, "Well, John von Neumann grabbed me and had me write a computer code." More evidence of that computer culture came from George Menschen, who wagged his finger at me and said, "Tom, here we're not Thomas Edison. We do not test hundreds of filaments for a lightbulb to find out the best one. Here we model a theory on a computer first, so we know what is the best filament. Only then do we conduct a test to verify our calculations." And that kind of spirit and environment was very, very rich at Livermore, and I loved it. It was energizing. It made you feel like you had power, if you will. The term empowerment has become a cliché, but at the Lab it was real. You felt like your ideas mattered. If you said something, someone listened to you. I can be wrong, but I feel like that kind of ethic has eroded a bit over my tenure at the Laboratory.

Zierler:

A retrospective question as we get to the end of our discussion. As you say, you've had three acts at Livermore. Where do you see your greatest contributions, both administratively and scientifically?

Ramos:

Wow. I've asked myself that very question, and the answer I've come up with is, it's like asking which of your three kids do you like the best? I love all my kids. So it's the same way with my three careers. I really am proud of the fact and love the idea that I was part of a program that I invented the most brilliant laser in the history of mankind. I'm very proud of that. And I'm also very proud with the CAPS program that we must've saved thousands of lives by telling people not to do things, not to indiscriminately bomb stuff, but to be clever. And I know there are people alive that wouldn't be alive today if it wasn't for that CAPS program. I'm very proud of that and what we accomplished over two wars in Iraq. But someone came up to me recently and said, "Tom, this is the highlight of your career, that history book." I'm excited that I'm going to have a history book published that I've written, which I think will make a significant contribution to what we know of the Cold War years. I think there are things in that book that historians didn't realize. I've gone through the publication process, and I've had some highly distinguished historians review it who have said, "Tom, I've learned something." Your question is like asking me which of my three kids do I love best. I love all three, I'm very proud of each of my three careers and they've had very different outcomes.

Zierler:

When the book comes out, I'll get you ready for the question that many are going to ask. What is the one thing that you want to convey? You have such an insider's knowledge of Livermore, and yet, the audience is going to be people with only passing understanding of what has happened there over the years. What's the one thing that's most important for you to convey about what Livermore stands for and how that mission has changed over the decades?

Ramos:

I think what the Livermore laboratory stands for, and I've grown to love the institution, is a dream that Ernest Lawrence had. I've grown to greatly appreciate that man. I think the laboratory made significant contributions to the defense of this nation, especially during the Cold War. I mean, significant-its efforts had game-changing significance. Even Hans Bethe admitted that, as negative as he was to the Laboratory over the years. Livermore made a profound impact on the Cold War for the good of our country and that's a legacy of Lawrence, principally, but also von Neumann, and John Wheeler: those three gentlemen. One of the three chapters of my book is titled The Legacies of Lawrence, von Neumann, and Wheeler. Many historians, and therefore many readers, think the Laboratory started with Teller. I once got called up to Nuckolls's office because, "Tom, I heard in your lecture you were not saying good things about Edward." I had to explain to John that the Laboratory had been created by Lawrence, pure and simple. I gave him a list of source books that I used to conduct my historical research. To Nuckolls's credit, he read my sources and told me, "You're right." Those three giants of science helped to create an institution for us, and they showed us a way to get some of the brightest people in our country to collaborate and work together for our own social benefit. That was a great achievement: the Laboratory's legacy.

Zierler:

Last question, looking forward. Using the powers of extrapolation, both personally and as a representative, in many ways as a public face for Livermore, where does the Lab go next, and what's next for you?

Ramos:

I think the Lab can continue to play a significant role at the core of our national defense. The reason I say that is because over three quarters of a century, it's built up a phenomenal foundation on which to conduct further research. We have the wherewithal to do outstanding science, and we still have bright people around here. We have a new director now, Kim Budil, and she's already stated her goals to get the Laboratory involved with the national security of the nation. Very much like Herb York did back in 1952, when he had to come to grips with the world's situation, that's the future of Livermore. Get some really bright people together to work on solutions. Not that I'm a super bright person, but I'd be happy to contribute to that effort. I wrote a biography of Johnny Foster, which has not been published, except within the Lab. I wish I had the book with me, I'd show it to you. But it's a hardbound book, which is my gift to Johnny. He had asked me to meet him and I said, "Sure. Come on over to my house, and we'll have some wine." When he was sitting out in that beautiful patio you saw before, we were sipping wine and he said, "Tom, I want you to write my biography." I looked around like, "Are you talking to me? What are you talking about?" And he says, "Tom, you understand what it is I accomplished, and I trust you." One does not turn down Johnny. I have grown very fond Johnny over the years. I can't count the number of times I've interviewed him, spent weekends at his apartment down in Santa Barbara. We talk about the future of the Lab quite a bit. I think we're in agreement that the Laboratory can still play a really important role, but it will have to continue to draw world-

class talent. As much as we all love physicists, they're not everything; you need political scientists, chemists, and engineers. They all contribute. We'll need world-class actors in all those fields, and some damn good leadership. I think it would be beneficial because we are entering some dangerous times.

Zierler:

You could say that we're in them right now, even.

Ramos:

Yeah. I see it. So yeah, that would be my thoughts.

Zierler:

Tom, it's been a great pleasure spending this time with you. I'm so glad we were able to do this.

Ramos:

Oh, David, it's my pleasure. And I know I talk a lot, so I apologize.

Zierler:

That's the whole point. I want you to talk a lot. If you're not talking a lot, I haven't done my job. So I certainly have. Thank you so much, Tom.

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Additional Persons:
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